CONTEMPORARY MATHEMATICS

453

Surveys on Discrete and Computational Geometry

Twenty Years Later

AMS-IMS-SIAM Joint Summer Research Conference
June 18–22, 2006
Snowbird, Utah

Jacob E. Goodman
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Surveys on Discrete and Computational Geometry

Twenty Years Later

Preface

This volume of survey papers reflects many of the topics addressed by the speakers of the 2006 AMS-IMS-SIAM-sponsored Summer Research Conference "Discrete and Computational Geometry—Twenty Years Later," held from June 18 to June 22, 2006 at Snowbird, Utah. This was the third decennial conference on the same theme.

The first, held at Santa Cruz in the summer of 1986, inaugurated the first volume of the new journal *Discrete & Computational Geometry*, whose birth arose from the increasing recognition that the old field of discrete geometry, which went back more than a hundred years and which had experienced a huge growth in the twentieth century, was intimately connected to the new field of computational geometry that had emerged in the preceding decade. This connection was first manifested when a few computational geometers found themselves working on some old and new problems in discrete geometry, and at the same time a number of discrete geometers became interested in problems arising in computational geometry. The 1986 conference was intended to introduce the leading researchers in both fields to one another. As in every marriage, there was at first the tension between mutual attraction and the wish to preserve one's own identity. The marriage ultimately proved a great success, however, as was evidenced by the rapid growth of this new journal (going from initially publishing fewer than 400 pages per year to the present 1440), and by the fact that its rapidly increasing readership reflected the growing interactions of the two fields.

The second SRC, held at Mount Holyoke in the summer of 1996, celebrated the tenth birthday of the new field of discrete and computational geometry. The researchers were no longer new to one another, but quite familiar after ten years of working together. The currents of that time can be seen in the AMS volume *Advances in Discrete and Computational Geometry* which emerged from that conference.

The most recent meeting, in Snowbird, coincided with the publication of the recent solution of the 400-year-old Kepler conjecture by Hales and Ferguson, which was presented (essentially as an "extended abstract") in the *Annals of Mathematics,* and fully written up in *Discrete & Computational Geometry.* This proof combined traditional mathematics and the use of the computer in new and surprising ways much more intimately than in the 1969 solution of the four-color problem by Haken and Appel.

The present volume reflects the current state of this by now almost "classical" field. The topics of some of the papers (those by Bárány, Demaine et al., O'Rourke, Valtr, and Zong, for example) would have been recognized by many of the researchers prior to 1986, while others would not have been seen as belonging to the subject at that time.

We would like to thank the authors for participating in this project, and the many anonymous referees without whose help it would have foundered. It has been a pleasure to help bring this volume to the public, and an honor to have been able to work with the distinguished authors of the articles before you.

Jacob E. Goodman
János Pach
Richard Pollack

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Musings on Discrete Geometry and "20 years of Discrete & Computational Geometry"

Branko Grünbaum

As DISCRETE & COMPUTATIONAL GEOMETRY (abbreviated to DCG in the sequel) turns 20, due to the fortunate effect of my longevity I can contemplate the development of Discrete Geometry over the last 50 years, and the role DCG played in that development.

The early development of Discrete Geometry, and of Discrete Mathematics in general, was fueled by the many easily stated unsolved problems that were circulated in the 1950's and 1960's. Prominent among these were the problems that Hugo Hadwiger regularly published in *Elemente der Mathematik*, as well as the many papers and talks by Paul Erdős that challenged the imagination of a generation of young mathematicians. A collection of 50 "Poorly formulated unsolved problems in combinatorial geometry" was put together in 1963 by Leo Moser. After 1977 his brother, Willy Moser, together with János Pach (since 1986) have repeatedly expanded and privately distributed the collection. These editions served as the basis of the more survey-like recent book "Research Problems in Discrete Geometry" by Brass, Moser, and Pach [4]. Another collection of unsolved problems was widely circulated in the 1960's in mimeographed form by Vic Klee [20] and led to a number of papers; it was meant to form part of a joint project with P. Erdős, L. Fejes Tóth and H. Hadwiger, but this never materialized. Instead, Hadwiger collected and expanded his problems proposed earlier in a booklet, coauthored with Hans Debrunner [17]; English and Russian translations, both including additional material, were prepared by V. Klee, and by S. S. Ryškov and I. M. Yaglom, respectively.

Over the recent years, Discrete Geometry—which originally consisted mainly of the theory of packing, covering and tiling—expanded vastly to include many other geometric topics, such as configurations of points, lines, pseudolines, planes, etc., oriented matroids, Helly-type results, the structure of polytopes, rigidity, linkages, Erdős-type distance problems, tessellations, geometric graphs, combinatorial complexity of geometric objects, geometric transversal theory, and many others. As mentioned by W. Kuperberg [22] in his review of [4], the lines separating discrete geometry from the theory of convex polytopes, combinatorics and graph theory became blurred.

A long-running department in the *American Mathematical Monthly* promoted unsolved problems, many of a Discrete Geometry nature. Similarly, some of the problems in a section of the journal Discrete Mathematics were of a Discrete Geometry nature. Another collection of Discrete Geometry problems that circulated for many years was that of Harald Croft. It was expanded into a well-received book [6] coauthored with K. J. Falconer and R. K. Guy. Vic Klee and Stan Wagon published an interesting collection of solved and unsolved problems [21].

The availability of great computing power and computer graphics has had an invigorating effect on many topics in Discrete Geometry, and has been wholeheartedly embraced by most practitioners. As with all new tools, new questions arose concerning the computational difficulty of various questions. This led to many of the advances featured in DCG.

Several developments can serve to illustrate the changed status of Discrete Geometry and Discrete Mathematics in general. One is the almost unimaginable deepening of the mathematics involved. Whereas earlier publications can be said to present the easy pickings in the fields they cover, the tendency of the more recent works is to tease out the finer and harder results. Many of the latter require very careful estimates and ingenious constructions.

In many branches of mathematics the past few decades have seen the solution of old problems that have stymied researchers for decades or longer. In the theory of convex polytopes, some 35 years ago came the almost simultaneous proofs of the upper bound conjecture by Peter McMullen [26], and of the lower bound conjecture by David Barnette [2], [3]. These advances served as the starting point for the deep and detailed study of various aspects of convex polytopes, many in DCG. But these developments have been in a certain sense very simple compared to other advances in Discrete Geometry and related fields. I have in mind the proof of the four-color theorem by Appel and Haken [1], and of the Kepler conjecture by Hales [18]. In both cases, the degree of complexity was such that reliance on a very extensive and sophisticated computational component appears unavoidable; as a consequence, checking the proofs has become a very major undertaking, with only few people having the resources and the inclination to verify all details.

However, it should also be noted that in some of the widely publicized advances in other fields (Andrew Wiles' solution of the Fermat problem, Grigory Perelman's work on Poincaré conjecture) the verification has become entangled in difficulties due not to the use of computers but because of extremely advanced and specialized results from a variety of other fields—to such an extent that even collectives of referees have been stumped.

Let me turn now to other important—even though less spectacular—advances in Discrete Geometry concerning topics with which I am personally more involved. It will be noted that this explicitly excludes a large part of the works published in DCG and devoted to various other aspect of Discrete Geometry, and to all of Computational Geometry, with which I am not sufficiently familiar.

The investigation of Venn diagrams was once considered as ending in the three circles made popular in very basic math courses. It has since blossomed into a very sophisticated geometric discipline, with connections to group theory, lattice theory and other branches. I flatter myself that this development started with my papers [10] and [11], see Figures 1 and 2. It is amusing to note that [10] was rejected by both the American Mathematical Monthly and the Mathematical Gazette, before being accepted by the Mathematics Magazine and then earning the Allendoerfer award of the Mathematical Association of America. Recent years have brought

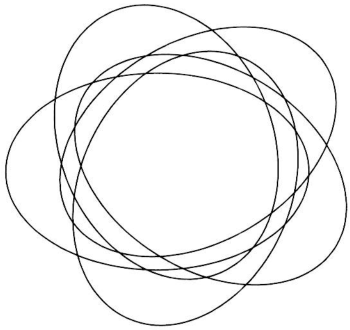

FIGURE 1

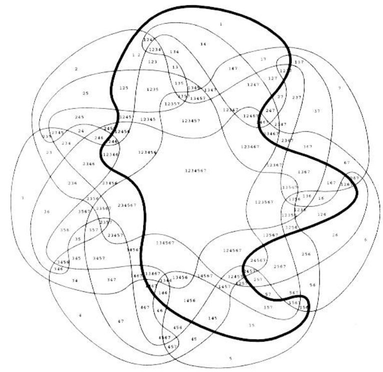

FIGURE 2

spectacular advances in the understanding of Venn diagrams, while still leaving many unsolved problems that are easy to formulate and understand. The extent of the development and changed status of the topic is best seen in the detailed survey given by Frank Ruskey [30] and the recent paper [29], the lead article in the December 2006 issue of the Notices of the AMS. But the outstanding question in the topic—whether simple symmetric Venn diagrams with 11 or more sets exist—is still open.

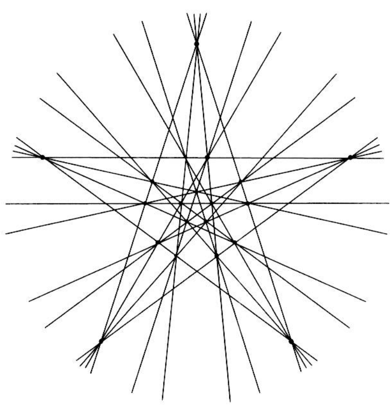

FIGURE 3

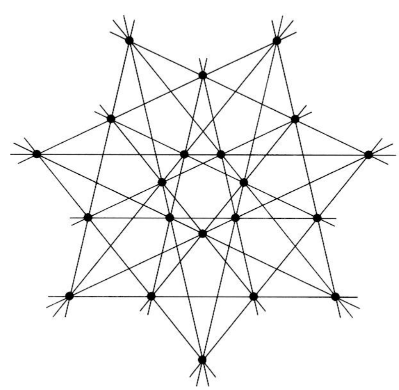

FIGURE 4

The theory of configurations of points and lines in the plane was somnolent for almost a century, despite the book by Levi [23], the chapter on configurations in the popular book by Hilbert and Cohn-Vossen [19], and several papers by Coxeter (in particular, [5]). More recently, the study of configurations took off due to several developments. On the one hand, the first ever diagrams of (n_4) configurations were produced [15], see Figure 3 (see also Figure 4). On the other hand, T. Pisanski and M. Boben recently found serious errors in basic results concerning the enumeration and construction of (n_3) configurations; these results were supposed to have been established long ago—in the nineteenth century—by V. Martinetti [24] and Steinitz [31]. Also, applications of computer algebra yielded the fact that for $n \leq 12$ all configurations possible in the real Euclidean plane are possible in the rational plane as well. Each of these directions led to many new investigations and unexpected results, as well as lots of open questions; a recent survey with detailed description of results and problems is [13].

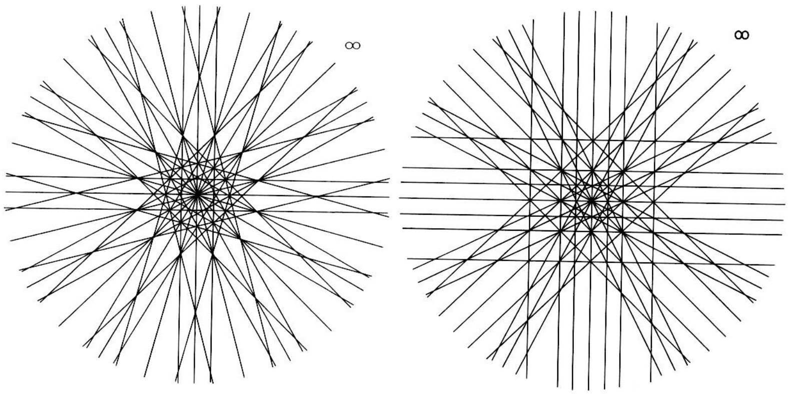

FIGURE 5

The theory of arrangements of lines in the plane, and more generally of hyperplanes in higher dimensions, went far beyond the simple questions considered since the times of Jacob Steiner nearly two centuries ago. Many extremal and other problems have been considered, and relations to algebraic geometry and other fields investigated. Among other open questions is the problem of determining all simplicial arrangements, still unsolved even in the plane; see Figure 5. Several recent surveys of arrangements (in the plane, and in higher dimensions) are available, together with indications of their use in various fields and many open problems. In particular, we should mention [7], Chapter 5 of [8], and parts of [28], [25] and [4].

The theory of tilings, in particular in the plane, has roots going to antiquity. More recently it has become quite popular, in part because of its relation to aperiodic and quasiperiodic tilings. Starting with [16], this has engendered many books and articles—several in DCG. Many of the publications are related to physical aspects.

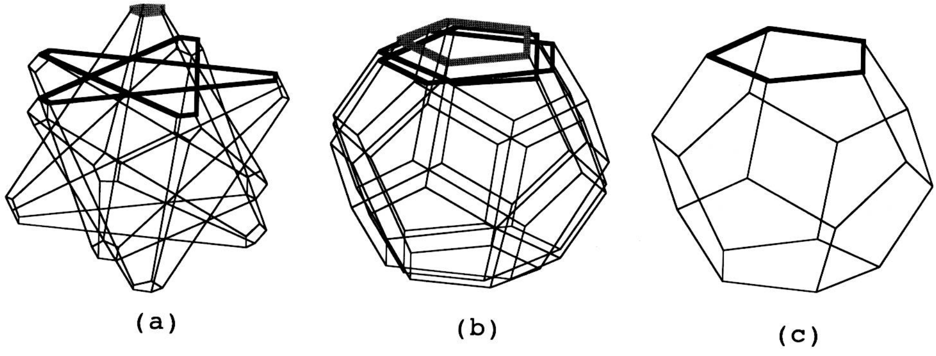

FIGURE 6

The theory of not-necessarily convex polyhedra in the Euclidean 3-space has also had significant advances. This topic stagnated since early in the twentieth century, and was revived towards its end. The renewed interest led to the consideration of several specific classes of such polyhedra, but more importantly it underlined the need for a consistent theory of polyhedra more general than the convex ones. This has now been developed (see [12], and Figure 6). It turned out to essentially coincide with the 3-dimensional case of the "abstract polytopes" of McMullen and Schulte [27]. However, the geometric reach of this work on abstract polytopes is limited by the insistence on what the authors term "faithful representations".

One additional new phenomenon is the widespread collaboration by multiple authors. While joint publications by two authors have long been an accepted feature in journals (as well as for books), the recent years have seen a surge in papers with three, four, or more authors. This is in part attributable to the ease of communication made possible by email and other electronic means. The possibility of quick interchange led to a much faster spread of ideas. In turn, this led to the many new approaches evident in the papers published by DCG and elsewhere.

Another new development that comes from the maturing of digital technology is the increased accessibility of a great portion of the literature. In many large research institutions (such as my home university) people enjoy almost unlimited access, free to the individual, to digital publications and repositories. In contrast, many of the workers at smaller institutions are not as fortunate. It is a sad fact that even the pricing of Mathematical Reviews (or MatSciNet) is imposing a heavy burden on people in such institutions. A similar inequality existed earlier, through differences in library holdings of various institutions. But one might have hoped that this would disappear, or at least be mitigated, in the digital age.

On the debit side of the proliferation of joint authorships and of papers in general one has to keep in mind the tremendous pressure on young researchers to come up with a long list of publications at the time of promotion and/or tenure, and even of primary employment.

In the 1950's there were no journals devoted to Discrete Mathematics. In fact, most journals were of a general character. The acceptable (and published) papers in this field were, on the whole, at a much lower level of technical complication and conceptual sophistication than has become the rule in later years.

As a side-effect of the increasing specialization of publications (and of mathematicians) several new journals were started, devoted mainly to Discrete Mathematics and some more particularly to Discrete Geometry. Unfortunately, this was accompanied by takeover of Enseignement Mathématique and of Geometriae Dedicata by editorial boards or publishers that were ignoring the original aims of the journals. The same is true for many conferences, such as the "Coxeter Legacy" where more than a half of the papers and presentations were less geometric than what Coxeter would have appreciated.

After surveying some of the directions of Discrete Geometry, a question that arises naturally is: Where is Discrete Geometry going? The only honest answer I can give is that I do not know. It is extremely hard to handicap the many emerging directions of investigation. For me, this uncertainty is increased by the very reason that led to the writing of this article. The longevity that gives perspective on the past implies, as a corollary, reaching old age. This, in turn, means a poor understanding of novel ideas and a regrettable tendency to see the future as a continuation in the tracks made in the past.

Finally, what about the future of DCG?

Excellent as the record of this journal has proved itself over the last two decades, I would venture to make three suggestions.

One is the active recruitment and solicitation of surveys of the different directions in which Discrete Geometry is actively developing. These should not be surveys written for popular consumption—readers unfamiliar with discrete geometry could hardly be expected to read them. Instead, the surveys should be authoritative accounts meant for generally knowledgeable people not specializing in a particular subfield.

Another is motivated by the availability of online versions of the published papers; this is certainly a step in the right direction. But the utilization of web-based possibilities could be enhanced by having a parallel online repository of detailed accounts of which only short reports would appear in the printed journal. This could be used for extensive tables or collections of diagrams, of accounts of proofs the length of which makes them unsuitable for the printed version. It could also be used for the surveys mentioned above, which in this mode could be kept up-to-date much more easily than in print.

Lastly, it is a fact that besides the academically oriented activities reflected in journals and meetings, there is a "parallel universe" of people communicating through the web, at a variety of levels of knowledge, but with a very high degree of enthusiasm. Many parts of the communications happening there are best left alone—because they reflect ignorance of well-known facts. However, the enthusiasm and energy invested in these web pages often contain genuinely new knowledge and interesting ideas and problems. It would be worthwhile to try to establish a connection with this universe, and make the interesting parts available to the academic community in the pages of DISCRETE & COMPUTATIONAL GEOMETRY.

* * * * *

The good fortune aspect of my long life was amplified by the acquaintance—and in several cases friendship—with many great mathematicians of the third quarter of the twentieth century, that had a more than passing interest in Discrete Geometry. This list would include H. Buseman, H. S. M. Coxeter, L. Danzer, A. Dvoretzky, P. Erdős, L. Fejes Tóth, W. Fenchel, H. Hadwiger, V. Klee, L. Moser, T. S. Motzkin, H. Rademacher, G. Ringel, I. J. Schoenberg, G. C. Shephard, and others, as well as many younger people that are still actively producing research mathematics. I'll never cease being grateful for their insights, inspirations, comments, and other kinds of support.

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State of the Union (of Geometric Objects)

Pankaj K. Agarwal, János Pach, and Micha Sharir

ABSTRACT. Let $\mathcal{C}$ be a set of geometric objects in $\mathbb{R}^d$. The combinatorial complexity of the union of $\mathcal{C}$ is the total number of faces of all dimensions on its boundary. We survey the known upper bounds on the complexity of the union of n geometric objects satisfying various natural conditions. These bounds play a central role in the analysis of many geometric algorithms, and the techniques used to attain these bounds are interesting in their own right.

1. Introduction

Let $\mathcal{C} = \{C_1, \ldots, C_n\}$ be a set of n geometric objects, such as disks or convex polygons in the plane, or balls, cylinders, or convex polyhedra in three and higher dimensions. Let $\mathcal{U}(\mathcal{C}) = \bigcup_{i=1}^{n} C_i$ denote the union of the objects in $\mathcal{C}$. The combinatorial complexity (or complexity for brevity) of $\mathcal{U}(\mathcal{C})$ is the number of faces of all dimensions on its boundary; see below for a formal definition. Several combinatorial and algorithmic problems in a wide range of applications, including linear programming, robotics, solid modeling, molecular modeling, and geographic information systems, can be formulated as problems that seek to calibrate the complexity of the union of a set of objects, or to compute their union. We begin by reviewing some of these applications.

Linear programming. Given a family $\overline{\mathcal{C}} = \{\overline{C}_1, \ldots, \overline{C}_n\}$ of n halfspaces in $\mathbb{R}^d$, we want to maximize a linear function over $\bigcap_{i=1}^{n} \overline{C}_i$. Since the maximum (if it exists) is achieved at the boundary of the common intersection, the problem can be reformulated as minimizing a linear function over the boundary of $\bigcup_{i=1}^{n} C_i$, where C_i is the (closed) halfspace complementary to $\overline{C}_i$; see Figure 1. The worst-case running time of the simplex algorithm, as well as many other naïve solutions to linear programming, is proportional to the total number of vertices of $\mathcal{U}(\mathcal{C})$. According to McMullen's Upper Bound Theorem

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